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IN TODAY'S CLIMATE, A FORECAST FOR CHANGE: A COMMENTARY ON DONAHOE, PALMER, AND BURGOS

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Donahoe, Palmer, and Burgos' essay raises several interesting questions concerning the future of the analysis of behavior, independent of whether neural networks ultimately turn out to be the potent biobehavioral models the authors suggest. The real difficulty that thwarts the authors' attempted convergence of behavioral systems appears not to be the conceptual nature of the S-R issue, but rather procedural and measurement differences that have evolved following the divergence of operant and classical learning traditions.

Skinner's conception of the operant never denied antecedent controlling stimuli, but only ones that were reliably observable. Rather than postulate their existence as a matter of first principles, Skinner ignored them and concentrated instead on the reliable relation at the other end of the behavior–environment interaction, the R-S relation.

> An event may occur without any observed antecedent event and still be dealt with adequately in a descriptive science. I do not mean that there are no originating forces in spon-

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> taneous behavior but simply that they are not located in the environment. We are not in a position to see them, and we have no need to. This kind of behavior might be said to be *emitted* by the organism. (Skinner, 1937, p. 20)

Skinner would probably replace "environment" with "publicly observable environment" today, but the emphasis in this sentence is the phrase "we have no need to." Skinner was marshaling the call for the analysis of reliable relations between behavior and environmental events other than antecedent stimulus relations, ones that prior to that point had been ignored in the models of the time. "The attempt to force behavior into the simple stimulus–response formula has delayed the adequate treatment of that large part of behavior which cannot be shown to be under the control of eliciting stimuli" (Skinner, 1938, p. 20). Again, the reference is not to behavior that *is not* under the control of eliciting stimuli, but rather to behavior that *cannot be shown* to be under the control of such stimuli. Rather than wait for some future time when (as in Donahoe & Palmer's, 1994, neural networks or other processes) eliciting stimuli could be implicated, Skinner adopted the utilitarian strategy of working with an altogether different set of correlations, those between responses and their consequences. Although he abandoned it later, he may have been closer to the mark in *The Behavior of Organisms* when he maintained that both classes be considered reflexes, differing only with respect to the reliable correlation (S-R or R-S) that defined the specific unit.

> The kind of behavior that is correlated with specific eliciting stimuli may be called *respondent* behavior and a given correlation *a respondent.* The term is intended to carry the sense of a relation to a prior event. Such behavior as is not under this kind of control I shall call *operant* and any specific example *an operant.* The term refers to a posterior event, to be noted shortly. The term reflex will be used to include both respondent and operant even though in its original meaning it applied to respondents only. A single term for both is convenient because both are topographical units of behavior and because an operant may and usually does acquire a relation to prior stimulation. In general, the notion of a reflex is to be emptied of any connotation of the active "push" of the stimulus. The terms refer here to correlated entities, and to nothing more. (1938, pp. 20–21)

Operants and respondents were different perspectives on the analysis of behavior, not mutually exclusive classes of events. When the most reliable correlation was between an antecedent stimulus and a response, the reflex was a respondent; when the reliable correlation was between a response and consequence, the reflex was an operant. Conceptually, then, Donahoe and Palmer (1994) converge on the system of behavior originally presented in *The Behavior of Organisms* by providing for control of behavioral units via either antecedent or consequent stimuli through a unitary mechanism.

Greater difficulties arise, I believe, from the fact that the procedures and methods that were subsequently developed to study operant behavior are not well suited to the development of a science of behavioral dynamics like that promulgated by Donahoe and Palmer (1994), as I and others have argued elsewhere (e.g., Galbicka, 1992, in press; Galbicka, Kautz, & Jagers, 1993). They also differ from those associated with the respondent learning tradition, where, with its emphasis on discrete presentations, response characteristics such as magnitude or latency, or aggregates such as response probability (i.e., responses per trial) are far more common dependent measures. The partitioned nature of respondent conditioning procedures also predisposes them to iterative, trial-by-trial dynamic learning models and processes (e.g., Rescorla & Wagner, 1972). The operant conditioning tradition, lacking any defining response cycle, abandoned the traditional measures and adopted response rate in their stead. This was not sufficient in and of itself to forge a bifurcation, however, because Skinner's use of rate was far different from that predominating today in the analysis of behavior. Donahoe et al. correctly emphasize that "Skinner was resolutely committed to a moment-to-moment account at the behavioral level of analysis" (p. 200). In a recent chapter, I argue this same point in considerable detail (Galbicka, in press). For Skinner, rate's primarily value was as a means of visualizing behavior *change,* as depicted in the cumulative record, not as a dipstick into the "reflex reserve" from whence to measure overall response output. Only when the use of rate was combined with the adoption of steady-state methodology did the analysis of behavior abandon behavioral dynamics for quantitatively more comfortable descriptive models of asymptotic behavior. Analyses of local patterns of responding, the conditioning and extinction curves so common in *The Behavior of Organisms,* became relatively rare. In their place, large aggregates of behavior relatively void of local structure and large aggregates of consequences delivered at unpredictable points in time predominate, and models of the ratios of these values across samples of thousands of responses rule the day (e.g., Davison & McCarthy, 1988).

For many, the quantification of behavior that has accompanied the adoption of this perspective signals the maturation of a science of behavior, and advocating the development of models like those proposed by Donahoe and Palmer (1994) might be considered a step back into the murky dawn from whence we came. But as Donahoe et al. note, "conditioning processes are instantiated in moment-to-moment relations between events" (p. 201), and regularities at more global levels of analysis are not invalidated by the development of a local model. They cite several examples in which local reinforcement contingencies are demonstrated to override more molar relations. Two additions to this list that I find particularly relevant involve studies on shock-maintained behavior, and ones employing Platt's percentile reinforcement schedules. I and others have demonstrated that the effectiveness of local relations can be so powerful as to confound the apparent function of stimuli at the more molar level, as when differential IRT punishment contingencies generate reinforcement-like effects in procedures in which lever pressing is maintained by contingent presentation of electric shock (e.g., Galbicka & Platt, 1984; Lawrence, Hineline, & Bersh, 1994). Stimulus function is not altered under these procedures (i.e., shock does not get transformed into a positive reinforcer; see Pitts & Malagodi, 1991, for a particularly elegant analysis); rather, the behavioral unit that gets differentiated is diametrically opposed to pressing, such that the functional effect appears to be reversed (i.e., by suppressing long IRTs through punishment, lever-press rates increase). Confusion in this case stems from treating all responses as identical members of the same aggregate (lever pressing) when, in fact, there is a differential relation (i.e., a contingency) that is sufficient to shape local patterns of IRTs. The fact that this relation is also present under fixed- and variable-interval schedules of reinforcement makes the simple identification of reinforcers and punishers impossible under such procedures.

Shock-maintained behavior is an extreme example of how local contingencies can override molar relations. Platt's percentile schedules (cf. Galbicka, 1988; Platt, 1973), which were developed to control molar reinforcement contingencies while independently manipulating more local ones, provide a second realm of research indicating that molar relations are not outcomes independent of the local contingencies comprising those relations. These data, in addition to those cited by Donahoe et al., prompt development of molecular models of behavioral processes, of behavioral dynamics, like those proposed by Donahoe and Palmer (1994). As the authors indicate, however, any such models must have as one solution at equilibrium, the molar relations, such as matching, readily observed under standard concurrent scheduling arrangements. As I have argued in the past,

> A complete model of behavior must ultimately be able to account for behavior change that is produced both by changes in overall reinforcement rates and in more local relations like the one programmed by percentile schedules. Perhaps it is time to change strategies and attempt to model the local dynamics of responding as they are related to local reinforcement characteristics, while keeping as a linchpin of any such model the requirement that it track the behavioral effects of changing aggregate reinforcement parameters as well. (Galbicka et al., 1993, p. 182)

Behavior analysis has until very recently avoided developing a "mechanics of the animate," to borrow Killeen's (1992) phrase, satisfying itself instead with descriptive analyses of steady-state performance. Attempts to do otherwise have met with resistance because, being new, they are necessarily incomplete, but also because they require a radically different view of the subject matter and a reexamination of response rate's value as a dependent variable. Reaction to the model proposed by Donahoe and Palmer (1994) is no less subject to controversy but no less desirable a course of action.

It may be helpful to consider a metaphor I used in discussing many of these same issues recently (Galbicka, in press). I noted that molar models stand in relation to behavior dynamics as "climate" does to "weather." Climate is an information aggregate predictive for large aggregates of time, but of little use either for dealing with today's forecast or for ferreting out the factors responsible for that climate. It is a summary of observations already made, not a mechanism capable of addressing day-to-day changes in the climate, which is termed *weather.* The weather is to be understood (predicted, if not controlled) from analysis of local changes in various meteorological factors (e.g., jet stream, wind and sea currents, air pressure changes, etc.). Weather can only be viewed as being responsible for the climate, not a product of it. In a similar fashion, models of behavior must at some point indicate not merely the aggregate value that responding will achieve (as current steady-state models do), but also the dynamic that allows it to attain that level.

To date, behavior analysis has functioned primarily as climatology. We have implemented schedules of reinforcement and described the resulting rates and patterns of responding, paying relatively less heed to the factors generating that behavioral climate. Donahoe et al., along with many others, wish to begin forecasting behavior change with considerably more precision than the simple ordinal relations that current definitions of reinforcement and punishment allow. Although quantification at this level must necessarily be cruder than quantitative models of steady-state performance (much like forecasting must be more variable than describing the climate), it is a necessary step in securing what is by rights the ultimate domain of operant and respondent conditioning: the analysis of variables that change behavior.

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